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Worms & Things

*The first issue (Vol. I, No. 1) of this prodigious journal is now history and, all things considered, can be considered a modest success. More than 200 copies went their errant way about the world, from Finland to Australia, to some 12 countries and about half of the 50 States. Herewith follows the second issue, not-so-hard on the heels of the first. We have decided that the end of the calendar year shall mark the beginning of a new volume. This seems not only logical, but also gives us the prestige of already being into Volume II and hence a journal which can make some pretense at having "endured".

Our distinguished colleague RALPH GERARD, one of the Cerebral Ganglia at Michigan's Mental Health Research Institute, informs us this journal should be called "The Worm Turns." We welcome his suggestion, but fear such a title might suggest a certain amount of levity on our part, and as our readers know, we abhor levity in science. After all, one might ask, "what is the worm turning into?" We have enough difficulty determining what the DIGEST may turn into, but we comfort ourselves that no matter what our future may be, we have had a glorious past!

One thing remains certain, however. We will remain a "free" journal in several senses of that adjective. There is no charge for a subscription, nor do we contemplate asking for donations. The DIGEST's costs, such as they are, are supported chiefly by the Department of Psychology. Our research is supported, in the main, by grants from the National Institute of Mental Health and from the Atomic Energy Commission--so both those august bodies should be considered partial "sponsors" of the DIGEST. We hesitate to hold them responsible for what we or other authors may wish to say in these pages, however, so we do not list them as "patrons" of the DIGEST on the front cover. We do thank them sincerely for their generous and timely support, however.

*Many of the copies of the first issue went to various high school students across the country who had read the story JOHN BARBOUR had written about the worms for the Associated Press, or saw the story in NEWSWEEK, and who wanted to pursue planaria as possible Science Fair Projects. Each time we sent these aspiring scientists the information they requested, we crossed our fingers and hoped they would not encounter too many difficulties in attempting to condition their experimental animals. It is no easy thing to undertake a project using an animal you haven't learned to live with, despite the apparent simplicity of the experiment itself. Simplicity is not one of the earmarks of science, however, and perhaps we do the younger generation a dis-service by suggesting that almost anybody can sit down and run a meaningful experiment "over the week end," or build a do-it-yourself cyclotron in his basement in a few days. Our journal reports seldom mention the heartaches (not to say the backaches) that go hand-in-hand with good experimentation. Only someone who has attempted a laboratory study from beginning-to-end can appreciate the First Law of Experimental Science: "Everything that can go wrong with an experiment, will."

Our worst fears along these lines were realized recently when we received a letter from a Chicago high school sophomore. It was sent from his home (via

ordinary mail) on the 20th of February, reaching our desk the morning of the 23rd. He had read about the worms, and in quite a long letter told us how excited he was. Would we please send him all the information necessary to replicate our findings, and above all, would we please send him some worms (underlined several times)! Also, since his Science Fair Project had to be completed by the 4th of March, would we please hurry? He figured he'd have to have the animals (he never mentioned the necessary equipment) by the 29th of February to be able to meet the deadline. And to think it took us a long year and a half to complete our first study!

*Our spies report that ROY JOHN, at the University of Rochester, and his assistant, BILL CORNING, have succeeded in imbedding (if that's the proper word) planaria in agar. Once the beast is held fast, they implant electrodes and take recordings. We have not heard from JOHN or CORNING directly, so we cannot give further details at this time. Perhaps by the next issue of the DIGEST we can twist their arms and get them to answer our letters.

*We chatted recently with BOB REYNOLDS, a rising young colleague in Pharmacology here. He tells us there are "winter" turtles and "summer" turtles, and never the twain shall meet. That is, a given turtle may show one kind of pharamacological response in summer, perhaps an entirely different, opposite response in winter. For example, substances such as adrenaline may cause a marked reaction during one season, little or no reaction during another (as we remember it). The differences are so marked, BOB says, that experiments performed on these animals in the summer have to be repeated in wintertime. Putting a "summer" turtle in a cold room for several weeks is apparently not enough to turn it completely into a "winter" specimen. This seasonal change is found not only in many amphibia and reptiles, but also in some mammals, such as the WHITE RAT (all experimental psychologists, please note!). Frankly, we were delighted to hear of BOB's data, for it has become increasingly clear to us that there are "winter" worms and "summer" worms. In the colder months (at least here in Michigan), very few worms can be brought easily to the criterion we set for "conditioning." In the spring, summer and early fall, almost all worms can be. We are currently revising our experimentation schedule so that most of the actual conditioning will take place during the warmer months. Biological clocks, anyone?

*Speaking of such things, we hear from JAY BOYD BEST, at Walter Reed in Washington, that he has found something akin to a biological clock in tigrina. Even when his animals were kept for some time in his light-proof, air-conditioned lab, they still showed a diurnal food-taking cycle. JAY reports that tigrina will cannibalize members of a species which Carolina Biological Supply classifies as dorotocephala, but which JAY insists is really Curtisia foremani. The cannibalization reaches its height from 10 p.m. to 3 a.m. and is a marked phenomenon even when he controls for the tigrina's normal activity cycle. Cannibalization is most marked when the tigrina have been starved for at least five days. Look for a more complete report of this soon in our sister publication, SCIENCE.

As soon as we heard of JAY's results, we sent for some Carolina "dorotocephala" and are now engaged in conditioning same to feed to naive, but hungry, tigrina. Will we find a type of "digestive transfer of conditioning?" Read the next issue of the DIGEST to find out.

Cannibalization strikes us as being an easier feat to accomplish than attempting to graft a conditioned head to a naive tail. In order for grafts to "take", the two halves have to be completely paralyzed for at least two days, or they move apart and grow up into two worms instead of one hybrid. To date, we have been unable to find an anesthetic which will paralyze planaria without killing them. None of the "standards", including the new one, MS-222, seem to work. Putting the animals on an ice cube stops them nicely, but they tend to freeze if left on the cube for more than a couple of hours or so. ROMAN KENK, whom we think of as being "The Sage of the Library of Congress," and who is a distinguished worm runner of long standing, reported in the 1940's that he had gotten only 2 or 3 grafts to "take" out of the 150 he attempted, so perhaps we shouldn't be too discouraged. We hope to return to this problem with JOHN's results in mind some time in late spring, when April winds have melted the snow of our winter's discontent.

*JAY BOYD BEST has also been attempting to teach planaria a maze. Instead of having the animals run in a water-filled series of alleys with darkness as a reward (as ERNHART and SHERRICK did), JAY has the worms run along dry alleyways for water as their goal. Last we heard, he had some preliminary data suggesting that the worms could learn the maze under such conditions. If these first indications hold true, they should provide as much positive reinforcement for ERNHART and SHERRICK as for JAY's planaria.

*We are overjoyed to report that a bright and shiny, quite functional sink has just been installed in our Mason Hall lab. Up until now, we had no water input or output, and every drop we used (including a gallon a week for coffee) had to be lugged hundreds of yards from the nearest tap, up and down two flights of stairs by our Gunga Din, DAVE BOUSE. Then, when the water was used, it had to be lugged back to the nearest drain. Now, thanks to the good offices of our chairman, E. LOWELL KELLY, and Deans ROGER HEYNS and BERT THUMA of the College of Literature, Science and the Arts, we have not only running water, but compressed air as well. What a difference a drain makes!

*Speeches, anyone? It struck us recently that many scientists spend considerable time preparing talks for presentation at the various meetings of the learned societies, or for symposia or colloquia, which are given, applauded, and then consigned to the filing cabinet. For our own part, we think some of our best writing and thinking have gone into such speeches which, for various reasons, have never seen the light of day thereafter. Why not publish same in the DIGEST, we asked ourselves, now that we're an "informal" journal of <u>all</u> comparative psychology? The answer we gave ourselves was, "Fine, but will anybody send same?" All right, <u>will</u> anybody?

*Coming Events: BEA GELBER, of the University of Chicago, has organized a symposium on "Chemical Correlates of Learning" to be given during the Midwestern Psychological Association convention in St. Louis the 29th of April. ROGER RUSSELL, of Indiana, will chair the symposium. BEA will give a paper, as will our pleasurable colleague, JIM OLDS. The Editor of the DIGEST has been asked to join this distinguished group and has gladly accepted, but feels badly outclassed. We hope to have at least a summary of the proceedings, if not some of the talks themselves, in the next issue of the DIGEST.

*In This Issue: The cover design is again by REEVA JACOBSON, without whose artistic talents the DIGEST would not be half so colorful. Actually,

we had hoped to have the cover professionally printed in four colors from REEVA's design, but when the printer informed us the cost would be equivalent to the price of 3,000 planaria, or 6,000 cups of coffee, we changed our minds. We apologize, too, for the fact that no one makes yellow ditto masters, but SALLY BOUSE has done a magnificent job of adding Michigan's Maize by hand. JOE JACOBSON, the DIGEST's cool poet laureate, is REEVA's brother and a psych major at Iowa State (K. SPENCE, please note)....PAUL and GRETCHEN CORNWELL didn't exactly meet over a worm trough, but they did spend many happy hours of their engagement running planaria together in our lab. This is the sort of togetherness we encourage. PAUL is now a graduate student in psychology here, while GRETCHEN, a recent Michigan graduate, is teaching school in Ypsilanti. We hope to lure her back to the worms during summers....MARGARET CLAY is about a year from her Ph.D. in psychology and acts as the Duenna of the Worm Lab. We couldn't do without her green thumb, for she can make almost any beast grow and prosper with her tender loving care (and, we suspect, a little black magic) ....GERRY ROTHSCHILD teaches the course in Comparative Psychology here at Michigan, but also holds an appointment with the Mental Health Research Institute. Most recently GERRY has been working with starfish. Maybe if you conditioned a starfish, then cut off one of its rays and let it regenerate....TOM MORRILL, our favorite evolutionist, lives in sunny Florida and spends more time reading, thinking and writing about the processes of evolution than anybody we know. His article on DARWIN which appears in this issue is a labor of love, condensed in part from an as yet unfinished book on the same general topic. It does not make for "light" reading, but in our opinion is well worth the time needed to give it a thorough study. We are quite proud we could talk TOM into letting us publish the paper in the DIGEST....There is also an editorial by the EDITOR. Why else be Editor if you can't publish your own editorials?

*Good reading!

James McConnell
Editor

TITLE: The Effects of Vibration as the UCS in the Conditioning of Planaria*

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In 1958 it was thought that, since a considerable number of planaria were being damaged by shock, it might be desirable to use a different UCS. In this study, vibration was employed as the UCS.

Subjects: 22 naive planaria (D. dorotocephala and D. tigrina).

Equipment: A small electric doorbell was attached to the end of the conditioning trough such that the hammer would shake the trough. No shock was used. With these exceptions, the equipment employed was the same as that used by McConnell, Jacobson and Kimble (JCPP, 1959, p. 1-5; see also "Apparatus," by Allan Jacobson, this JOURNAL, Vol. I, No. 1, pages 1-2).

Procedure: With the following exceptions, the same classical conditioning procedure was employed as that used by McConnell, Jacobson and Kimble:

One S was used as a vibration control, being given 390 trials of vibration with no light as CS. The other 21 Ss were given light as the CS and vibration as the UCS. Ss not reaching the criterion of 23 responses out of any 25 consecutive trials within 390 trials were discarded.

Upon reaching criterion, each S was cut in half and allowed to regenerate. After regeneration, each S was again cut in half and allowed to regenerate once more. The resulting F_2 Ss were then tested for savings.

Results: All Ss contracted to the vibration on almost every trial. The vibration control S showed no increase in frequency of response during the 2 sec. interval preceding UCS presentation.

Of the 21 experimental Ss, 8 reached criterion, 9 reached 390 trials without making criterion and were discarded, and 4 died or were destroyed. In this experimental analysis, then, 47% of the initial Ss reached criterion during initial training, while 43% of the regenerates reached criterion during re-testing.

The experimental results are presented in Table 1.

TABLE 1
Mean Trials to Criterion

	Initial Training	F_2 (With Orig. Head)	F_2 (With Orig. Tail)	F_2 (Completely Regenerated)
N	8	4*	0**	2***
Mean	251	195		222
S.D.	95	133		16

* 3 additional animals in this group failed to reach criterion
** 1 additional animal in this group failed to reach criterion
*** 4 additional animals in this group failed to reach criterion

Discussion: Apparently planaria can be conditioned to respond to light by pairing it with a weak vibration. However, using vibration instead of shock as the UCS results in fewer Ss conditioning initially, Ss taking more trials to condition and Ss showing less savings of the response in the F_2 generation (see McConnell, Jacobson and Maynard, Apparent retention of a conditioned response following total regeneration in the planarian, Amer. Psychologist, 14, 1959, 410 (abstract)).

*This research was supported by a grant from the University of Michigan Research Institute.

TITLE: Preliminary Report on Project "Glowworm"[1]

AUTHOR: Margaret L. Clay
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One of the current lines of endeavor at our lab these days involves various effects of irradiating planaria. So far we're still in the exploratory stage (that glorious state that too many researchers never get out of!), but perhaps a run-down on our "so far" progress will give you some hints about another of the numerous types of things that can be profitably done with the little beasties.

We felt that we should first re-establish the lethal threshold for planaria from X-ray irradiation. Some work has been done on this, originally by Curtis and Hickman (2). They showed that large doses of X-irradiation (exposure equivalent to twelve skin-units) destroy the planarian's power of regeneration by destruction of the formative cells located in the parenchyme. Chevchenko (1) placed the lethal threshold for these cells at 2500 to 3500 roentgens (r.). Wolff and DuBois (5) have varified this threshold level on Euplanaria lugubris adults (hereafter referred to as Curtisia foremani, per Hyman (3) and Kenk (4)). They used a tungsten anticathode X-ray tube at 60 kv, 4 ma., without filter, at a distance of 7 cm. from the source for 15 minutes. Since this level involves only the softer rays, we attempted to determine the threshold at a higher level, higher energies being more frequently used in current X-ray research. We applied a 250 kvp (kilovolts peak), 20 ma X-ray with a 1 mm. aluminum and 0.25 mm. copper filter added. We irradiated at a TSD (target-skin distance) of 40 cm., with an HVL (half-value layer) of 0.375 mm. copper.[2] Higher energy rays, in a minute animal like the flatworm, might be relatively either more or less effective than softer rays, since their high penetrating power might send them right on through the tissue, giving less absorption, yet would tend to ionize more when they were captured, increasing their damaging power.

As a first study we used mixed groups of Dugesia, both dorotocephala (used by Curtis and Hickman) and tigrina, as well as some Curtisia (used by Wolff and DuBois). Five groups of 15 worms were irradiated at 208 r. per minute for intervals from 2½ minutes to 12½ minutes. The groups were as follows:

GROUP	TOTAL DOSE	TIME IRRADIATED	NUMBER IN GROUP
1	520 r.	2.5 min.	15
2	1040 r.	5.0 "	15
3	1560 r.	7.5 "	15
4	2080 r.	10.0 "	15
5	2600 r.	12.5 "	15
6	0	0	8

[1]This research is being supported by a grant from the Atomic Energy Commission.
[2]We would like to gratefully acknowledge the assistance and cooperation given us by the University of Michigan School of Public Health, particularly by Mr. Robertson J. Augustine, Research Associate in Environmental Health, in connection with the techniques and equipment used in our X-ray procedures.

We believed that group 5 approximated fairly closely a level slightly above DuBois and Wolff's lethal threshold, beyond which all their worms died within 2 to 6 weeks.

Immediately after irradiation in celluloid test tubes, the worms were transferred to fresh water in white opaque bowls 4 in. in diameter such as we regularly use in our lab. Each group was maintained in a separate bowl. Since one of these bowls comfortably accommodates from 40 to 60 worms, the quarters are amply spacious for a group of 15. The animals were fed brine shrimp the following day, and for two weeks thereafter received fresh water every day and brine shrimp every other day. After this period they were cleaned and fed regularly twice a week as are the balance of our animals. At the beginning of the second week a sixth group was added, the control, which was treated and counted from then on in the same way as the irradiated groups.

For the first two days all animals appeared fairly healthy. The first signs of illness showed up on the third day when five worms in group 5, four worms in group 4, and three worms in group 3 appeared diseased. By the next day, some deaths had occurred in groups 2, 3 and 4, and all bowls contained several sick worms. Losses occurred daily after that until, at the end of the second week, all remaining animals appeared healthy and vigorous. At this time there were seven animals in group 5, six in group 4, seven in group 3, seven in group 2, eight in group 1, and eight control animals. Conditioning procedures were begun on two animals from each group. Two days later some of the worms being conditioned began showing head malformations and black spots (usually located in or near the head region). We had to stop running these animals because they exhibited continual agitation and body contortion, yet showed much reduced motility in characteristic flatworm gliding activity. This later illness seemed to come on more slowly. Typically, the pharyngeal area humped up, and the worms gave the impression of slowly congealing while still alive. One worm lived for eleven days after the first signs appeared before he disintegrated, but none recovered, once signs of illness appeared.

By one month after irradiation, all animals were dead except seven in group 1 and the eight control animals. The early deaths appeared to be a function of an increased susceptibility to usual flatworm diseases, the animals showing signs much similar to those observed occasionally in stock animals which get sick and rapidly disintegrate to nothing. We suspect that the later deaths resulted from inability of the worms to regenerate new tissue. This is indirectly supported by data we collected on tail dropping, the usual form of reproduction (agamic) exhibited by these species. In the characteristic agamic process, a flatworm will, with a frequency determined by environmental conditions and species, separate himself from his tail at a point approximately 1 mm. behind the pharynx. The dropped tail, in a period of several days, will regenerate into a new flatworm, while the parent (?) worm will have grown a new tail and perhaps will shortly repeat the tail dropping process.

We kept track of tail drops in our animals as an index of retention or inhibition of their regenerative ability. The first tail drop among irradiated animals occurred in a group 3 animal [illegible] days after irradiation. Subsequently both the parent animal and the tail died. There were no tail drops in groups 5, 4, and 2. The first tail drop in group 1 appeared 25 days after

irradiation, the second 3 days later, and the third after another 12 days. By 50 days after irradiation this group of seven worms had thrown eight tails, all of which regenerated in about average time. Since then tail drops by group 1 have appeared at about the same rate as in the control group. In a period of 36 days, the controls (N = 8) threw 15 tails.

Forty-eight days after irradiation, we cut two worms from group 1 and two control worms transversely through the pharynx and compared rates of regeneration of the eight pieces. The rates appear to be almost identical, all parts surviving and totally regenerated within about a week. By this time, each of the four original head ends had thrown a tail. Therefore, twelve days after the original cut we had twelve regenerated flatworms, reduced in size, but fully formed, in place of the original four. No differences in appearance of the animals of the two groups were observable under magnification with a dissecting microscope. We intend to keep the irradiated colony for an extended period to check whether mutations begin to occur after several generations, but so far none have been observed.

Tentatively then, we can say that the lethal threshold under our conditions of irradiation appears to be somewhere between 500 and 1000 r's. At the lower dose, regeneration seems temporarily inhibited but returns to normal by about six weeks. We will probably be able to pinpoint both lethal threshold and regeneration rates more closely as we irradiate more worms at narrower intervals between our two crucial limits.

We're still collecting the data on conditioning from these animals, running them in a light-shock procedure involving comparative performance between groups over a block of 600 trials given in daily sets of 25 trials, the first 100 trials with light alone, and the last 500 using light paired with shock. Therefore, we're not yet ready to predict radiation effects on conditioning. But you'll be hearing from us.

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TITLE: On the Tyranny of Exact Measurements and Inexact Definitions

AUTHOR: James McConnell
Editor
Worm Runner's Digest

One of the many difficulties a scientist faces when performing experiments in a relatively "new" or "uncharted" area of science is that his laboratory findings often come into head-on conflict with his colleague's established modes of thought. When data and beliefs are in disagreement, the data will usually win--in the long run. But it takes considerable time for the battle to run its course and the fighting is often quite bloody. Scientists are supposed to have open minds--but this, of course, is merely one of the many folk myths in our culture. Scientists are people, and all people, scientists included, suffer from a malady called "cultural lag". Conservatism has its place, quite naturally, even in science. This is little comfort, however, to the experimenter who has followed all the rules of good experimentation and still finds his work disbelieved and in disrepute simply because his colleagues prefer their prior opinions to his newly-established facts.

Take the case of "learning," for example. All of us have at least a rudimentary notion of what the term means. In rather general and operational terms, we consider it "a (relatively permanent) change in behavior (or thinking, performance, cognition or perception) usually brought about through Practice (or experience)." There is absolutely nothing in this definition which states explicitly what sort of organisms can learn, nor what part of them learns, nor even what the internal mechanism of learning is (biochemical, neurophysiological, etc.). But hidden away in our own personal definitions of learning are implicit assumptions about who can learn, where learning takes place, and even what the mechanisms must be. These implicit assumptions have no bearing on the explicit definition--they neither lead to the definition nor follow from it. Yet, when we review new data from various laboratories, primarily we test these data not against the explicit definition but rather for their congruence with our "subterranean" beliefs. Do we hold the implicit notion that learning is primarily cortical? Then, of course, decorticates or animals so low phylogenetically that they lack cortical structures are a priori incapable of learning. Don't bother to bring around your data on spinal conditioning--"everybody" knows it's impossible. Do we hold the unconscious belief that a nervous system is necessary for learning? Then, of course, all that data on behavioral modification in paramecia must be wrong, not because it was obtained in an unscientific manner but rather because it contradicts our hidden assumptions. And don't dare mention the notion that plants might show some evidence of "learning" were we only astute enough to give them the opportunity! Perish forbid! A plant's learning? Absurd!

In like fashion, what do we mean by "conditioning"? Is it a method, a type of learning, the only type of learning, or a process of discrimination? Does it exist inside the animal, in the experimental procedure, or in the theorist's mind? Which? Well, let's look at "backward" conditioning for a moment. If it works, it should result in a conditioned animal whose responses were indistinguishable from those of an animal given "forward" conditioning where the CS preceded the UCS. Therefore, "backward" and "forward" conditioning

are the same thing. If you disagree with this line of reasoning, then, for you, conditioning is a procedure and not a form of learning. For isn't learning something that takes place inside the animal?

Or take flatworms, for another example. When we pair a CS with a UCS for sufficient trials, and the worm begins to respond to the onset of the CS alone, is this conditioning? If conditioning is defined as whatever response modifications occur following CS-UCS pairing, then the worms are "conditioned". If we must further prove that the animals could discriminate the CS from all other possible CSs before you are satisfied that the worms are "conditioned", then your implicit assumptions about conditioning include a "discrimination" clause. If it happens that the worms "generalize" too much, that they give the "conditioned response" to too wide a range of stimuli, then, of course, the worms have merely been "sensitized" and not really "conditioned" at all. It matters little that prior to the training, the worms would not give this response to those stimuli, nor that they will continue to so respond for months afterwards. Nor does it even matter that this responding carries over into further asexual generations. No, the beasts were "merely" sensitized, and that just goes to show they weren't conditioned after all.

Sensitization? How shall we define that term so that it is distinctly different from our general definition of learning? After all, it seems to be a (relatively permanent) change in behavior brought about through experience. We might even ask if all learning, including discrimination, does not involve some sort of sensitization? Is discrimination something above and beyond sensitization, a sort of higher-order conditioning, or does it differ from sensitization only in quantity and not in quality? And what is the difference (if any) between "sensitization" and "stimulus generalization"? Are both things a type of incorrect responding, or a responding to a broad range of stimuli? Ah, you say, but "stimulus generalization" operates along a clearly defined physical dimension, while "sensitization" is a responding to any old stimulus that comes along. But wait a minute. In the final analysis, all physical dimensions are learned by the organism, and anyhow, it is only from the animal's inappropriate responses that we know that it perceives two stimuli on a given physical dimension as being similar. So if the animal responds naively to a green light just as it does to a conditioned 1000 cps tone, is it responding just to any old stimulus ("sensitization") or does it perceive green lights as being on a physical dimension with 1000 cps tones ("stimulus generalization")?

I don't think we can be sure of solutions to these problems as yet, but I am sure that our current inexact and incomplete definitions of the terms won't help us much until we're willing to drag our hidden assumptions out and look at them closely.

Another difficulty the scientist faces when charting unexplored realms is that of being "scientific enough" in specifying what he did so that his colleagues will invest enough faith in his results to attempt to replicate his work. We probably have as many implicit assumptions about what science is as we do about what learning is. For example, science might be called a body of knowledge, or a method of obtaining knowledge. We would probably all agree that scientific knowledge is, perhaps above all else, precise. But how precise is precise? Must all physical measurements be to the nearest millimeter or microsecond? Are there not times when such attempted precision is not only misleading, but also flatly impossible to obtain? Probably we all

know of cases where someone has "estimated" distances or times to the nearest tenth of a unit, then squared the estimates, squared them again, and come out with "guestimates" which pretend to be accurate to four decimal places. Is this good science?

More to the point (for this journal), let us consider the worms again. It is rather difficult for someone watching worms "perform" in a trough to indicate precisely what the response was in standard c-g-s units. Such responses just don't "measure" that way. Perhaps some day we'll invent a Skinner Box for planaria, but is such quantification really that much more meaningful than counting responses observed visually? More reliable, perhaps, but is it more valid at this stage of the game? The question remains to be settled. But since exact quantification usually comes only after qualitative differences have led the way and provided us with phenomena to be quantified, perhaps we should not insist on too much precision from experimenters working on the perimeters of knowledge.

Nor should we insist on precise specification of laboratory techniques when such would only be misleading. When we "condition" worms, we do so by passing an electric current through the water they're in. The current comes from a 6-volt DC power supply and passes through a standard inductorium before it goes through the trough. Several people have written to ask us about the magnitude of the current and have implied we were rather remiss in not specifying in print what the amperage was. We have written back that in our case, precise measurement of current flow is not only impossible, but also that we would be misleading them if we pretended we could obtain meaningful measurements. As a matter of fact, we have only a vague and general notion of how much current the worms are receiving, and at the moment, we couldn't care less. This may sound shockingly unscientific, but follow our reasoning for a moment.

The important thing about DC current passed through an inductorium is that it causes the worms to contract yet does not prove to be latently fatal to the animals. AC current must be so intense to evoke a contraction that the worms soon die. DC current not passed through an inductorium usually causes the worms to "curl up at the edges" rather than contract. This much we have stated in print. But how much DC current? Well, we know we're putting 6 volts into the inductorium, and it doesn't seem to matter whether it comes from a power pack, a dry- or a wet-cell battery, or from a simple doorbell transformer. We could measure the amperage at the power source, but the inductorium changes this, so why bother? We could measure the current delivered to the electrodes in the trough, but the water changes that, so why bother? We could measure the flow through the water, but it is the current reaching the worm that is important, and the worm's resistance affects that, so why bother? And we can't measure the worm's resistance, so where does that leave us? Different worms would surely have different resistances, and since we use different water every day, with slightly different electrolytic properties, any attempts at precise measurement of current strength seem doomed to failure before they begin.

So rather than specifying the strength of the current in milli-amps, which would be both misleading and inaccurate, we do the next best thing. Or perhaps even a better thing, come to think of it. We specify behaviorally how much current to use. With each apparatus, and with each worm, we test

for a behavioral threshold. That is, we start with so little current that the worm does not contract, then adjust the inductorium to the point where the intensity of the UCS is just sufficient to obtain a clear contraction 100% of the time. This setting varies from one apparatus to another, from day to day, and from worm to worm. Surely anyone attempting to replicate our work would have inductoria, electrodes, wires, worms and water slightly different from ours, so the only way they could hope to get our results would be to duplicate the animal's response, not the magnitude of the current we happen to use on any given day. Is our approach precisely unscientific, or merely scientifically imprecise?

Implicit assumptions can be as lethal to creativity and the gathering of new knowledge as AC current is to flatworms. The creative person is therefore warned: If you must be original, disguise your originality as much as possible. Don't speak of "learning" in lower organisms, rather speak of "behavioral changes or modifications". You don't run afoul of subterranean beliefs about conditioning as much with the latter as with the former. Obscure your necessary inexactness of measurement by specifying even unimportant things to six decimal places whenever possible. Invent machines which will look frighteningly scientific, then incorporate them into your apparatus even if they won't quite do the job that a human observer would. Nobody can argue with a complex circuit diagram they can't decipher anyhow, but everybody will argue with "personal observations". If you find something really astounding, something so startling that it contradicts all current theories and runs rampantly athwart of most people's implicit assumptions of what the world should be like, be sure to write up your experiment as if you didn't believe your own data, as if you were convinced that something must be wrong with it, but, well, here it is anyhow. Then, perhaps, someone will believe you.

But if you want to be safe, secure, well-paid and soon-promoted, stick to the tried and true, do what everybody else has done--only do it more precisely! And for heaven's sake, don't try to condition plants! They might just be capable of learning, and where would that leave our implicit assumptions?

TITLE: Darwin's Forgotten Theories

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During the centennial of The Origin of Species, when much attention is being paid that book's concept of evolution, it might be well to recall that Darwin also proposed and stoutly defended two other, quite different theories. "Natural Selection" was well on its way to becoming "the theory of evolution" when the great theorist published his contradictory hypotheses of "Pangenesis" (1867) and "Sexual Selection" (1871). Both these latter concepts rested on the far-reaching assumption that the organism and its surroundings are a developing whole, that heredity and environment are co-extensive with the organism. I would like to discuss some recent findings which suggest that Darwin was as profound in his all-but-forgotten other theories as in that of Natural Selection.

The well-known theory of Natural Selection regards plants and animals as being essentially inert, struggling merely for "existence" and "survival", forced to evolve by accidents of heredity and fluctuations of external environment. In present day language, heredity is changed only by "higher temperatures and energetic radiations...(that heighten) the 'molecular chaos'" (Muller, 1951). Such "gene mutations," as they are called, are summed into new species by "adaptation to environment...the guiding force of evolutionary change" (Dobzhansky, 1956). An oft-cited example is the darkening of certain species of moths in Europe within the past century. Darker variants, less likely to be eaten by birds in the industrially-blackened woods, are thought to have arisen purely fortuitously from random mutation, the sooty living having nothing to do with the case. The selecting soot and the birds are of course "environment," therefore quite out of the moths' province.

Darwin's other theories explored the possibility that heredity and environment are unified by the organism, Sexual Selection by means of psychological processes, Pangenesis by means of physiological processes. Under the theory of Natural Selection, evolution comes about from accidents in genes much like water gushes randomly from a spring, then is channeled away by the environment. The two other theories see evolution as continuous and self-regulated--as water rising from an ocean and returning. According to the theories of Sexual Selection and Pangenesis, the organism is a "gene-shed" no less than genes are "organism-springs". As a tree's growth condenses sunlight in wood, so a species' evolution condenses behavior in genes; fertilization releases life as a match releases the sun.

The later theories of Darwin are at base the last expression in western science of those old, fond dreams of men--that in its largest aspect, beyond the misery, grime and cataclysms of earth, life is a "striving towards a goal, a far circuit and a sure coming home."

Sexual Selection

In the Origin, man's choosing lines of domestic breeding stock was seen as an analogy for Natural Selection. It was not evidence and example because

"directive Nature" was assumed to be organism-external-and-autonomous; wild animals were selected without being selectors. But should man be merely analogy, cut off from the other vertebrates which are presumably his ancestors or descendents with him of common ancestors? Since "animal choice" is paramount in the most evolved species--and man is the first and the most domesticated breed--would it not also direct evolution in other species? So runs Darwin's argument for Sexual Selection in The Descent of Man.

Sexual Selection's title is misleading. Under it Darwin really is talking about animal selection; a full, descriptive title would be "female animal, esthetic selection". Thus the marvelous designs and colors of butterflies, birds' colors, plumes, ruffs and crests, are assumed to come from the females' pursuing (over many generations) certain "standards of taste" in their choice of mates, yet "appreciating slight changes" in lines of development. The fallacy in this argument is in the peculiar limit Darwin put on the principle. If animal choice is a factor in evolution (and nothing seems more certain), why restrict it to females' choosing nicknacks in males? Domestic breeding itself has almost every imaginable object--except sex. What is there which is "ornamental" or "sexual" about a Burbank potato?

Would not animal selection by males as well as females, include choice of companions-other-than-mates, habitations, whole ways of life? Though repeatedly suggested in these more logical forms by respected scientists, animal selection has never caught on in the face of an over-whelming preference for environmental selection of hereditary accidents. One reason for this perhaps is our own culture pattern: our culture is other-directed and accident-prone, causing its members to seek such an irrational rationale in the universe. Also, the phenomena of animal selection are very complex.

Consider bees pollinating flowers. Steadily over thousands of years and generations, flowers which bear increments of a certain fragrance, color, design, are pollinated more than other variants; pollen from flowers having a desired fragrance is crossed with flowers having an attractive color; finally these buzzing, fussy, persistent little Burbanks succeed in breeding a new clover or snapdragon. This is actually how students of the evolution of flowering plants think many species originated. The forgotten question is: What directs the bees as they direct the flowers? The problem to be faced here is an enormous one, and not to be dismissed by science because of the difficulties involved in its study. The problem is as fascinating and significant as it is difficult of solution. We face the problem in domestic breeding; we see it again in art--how does a school of painters develop a certain style and palette over the years? This much is clear: each new generation of flowers, each new painting or "period," reacts on the selective apparatus, producing more of the same. The leading student of the mutual evolution of insects and flowers, E. E. Leppik, writes:

"In all probability there are definite series of inborn instincts and inherited reflexes in...(pollinating) insects which function automatically as responses to the usual stimuli produced by certain flower types. Some reactions in these insects seem to be acquired as a result of experience "learning," and could be classified as conditioned reflexes. Obviously such conditioned reflexes produced by some usual stimulus repeatedly (presented) over a long period of time, finally become firmly established in the nervous system of insects as inherited reflexes. Many reflexes are known to be interrelated, so that a reaction to a certain stimulus may act as a stimulus for

a second reflex, this for a third one, and so on until the full series of definite actions is completed" (Leppik, 1957).

Many persons before Leppik have analysed this problem, giving it a pompous name, "projection-identification". Children and even worms do something like it in their "leap-frogging" and "inching". Mountain climbers drive pins into rock faces which then bear their weight; perhaps teams relaying upward supply caches, each mounting the next ascent, is more comparable. Over series of generations, the process implies coordinated development in genes. The flowers the bees pollinate might change by gene mutations, but mutations in the bees would disrupt and distract the process. The problem is a major one--indeed it seems the major one--in physiology as it is in psychology. The title of a paper by Verplanck (1955) asks, "Since innate behavior is learned and vice versa, what now?" A basic issue would seem to be a biochemical theory of inheritance. Darwin's second forgotten theory points the way.

Pangenesis

> Every part of the body is imagined to throw off innumerable particles called gemmules, which getting into the blood stream are supposed to collect in the germ cells, and these combine with like units already present, modifying them in accordance with the changes that have taken place in the periphery. (Encyclopaedia Britannica, "Lamarck".)

Pangenesis is not as naive as was believed when it was announced in 1867. Body cells indeed contain molecules "like units already present" in germ cells: "gene" is a shortening of "pangene," applied "in honour of Darwin" by the founder of the mutation theory, Hugo De Vries (1922). To complete the emergence of Pangenesis as a modern theory, we must demonstrate three things: (a) that genes change from physiological molecules and processes; (b) that body cells secrete characteristic molecules; and (c) that transmission can take place between somatic cells and germ cells. On points (a) and (b), there is conclusive, positive evidence. On point (c), that somatic-germ transmission can take place, we will have two suggestive cases.

(A)

Until five years ago, genes had been to biology what atoms were to Newtonian physics. They were permanent resting points, discrete essences, unmoving movers. While still awaiting its supreme release comparable to the atom bomb, biology now has its great Rutherfords who have absolved its "atoms" in the system and process of life. Observed in protozoa by 1947, "nuclear differentiation" yet remained like those sea serpents and demons off the edges of old maps until 1955-6, when King and Briggs made it a central landmark.

Differentiation of somatic cells in metazoa, say from zygote to bone or zygote to nerve cells, seems to require that somatic genes change in order to direct the process. Even if this were not so, somatic genes would seem to have to change from the prodigous changes in the milieu. In keeping with the prevailing Weismann-De Vries theory, science long thought that somatic genes were identical with germ cell genes. Thomas King and Robert Briggs removed nuclei from frog eggs, replacing them with nuclei from the gut of

frog embryos. The resulting embryos had well-developed endoderm parts, but their nervous systems were deficient or lacking. When altered nuclei were serially returned to fresh eggs five times and grown out through 65 cell reproductions, the alterations continued undiminished. Fischberg, et al. (1958), after repeating the King-Briggs tests, and also having noted irreversible changes in nuclei reciprocally transplanted between cells of different frog species, conclude that the changes occur "probably...at the level of genes." They cite their own and a number of other experiments for the statement: "There is therefore no doubt that the cytoplasm can change nuclear qualities." The mechanism for all this is apparently similar to that of "projection-learning": cell lineages form in step-by-step elaboration-reaction with their own products. This view has firm support in work now being reported.

All genes, in any organism whatsoever, are built around the chemical, deoxyribonucleic acid. On such "DNA" is molded the "RNA" (ribonucleic acid) which, entering the cell cytoplasm, synthesizes proteins. However, RNA is also developed by organism response--enzymes and antibodies are produced by RNA modified in specific reaction with foods and antigens. The findings of Niu (1958), coupled with those of his fellow embryologists King and Briggs, suggest that organism-modified RNA specifically alters DNA. When RNA refined from kidney, and RNA from thymus, were added to undifferentiated embryo cells, kidney grew from cells in contact with kidney RNA, thymus from thymus RNA. That modified RNA is a feed-back precursor of DNA is also indicated by much recent work on the nature of mutation; rather than being direct damage to genes, mutation now appears to stem usually from injury in, and copied from, RNA.

Another form of physiological gene change occurs in organisms bearing genes damaged by mutagens, i.e., X-rays, ultraviolet light and nitrogen mustard. Damaged genes are repaired ("re-generated") the same as cut skin. There are five studies cited in the bibliography dealing with this issue (Blum, et al., 1951; Bunting, 1951; Guthrie, 1949; Lindegren, et al., 1957; Wright, 1953). Of these five, Wright's seems to the author classic in everything but its recognition--it has been ignored like another classic paper, Mendel's before 1900. The common flora of the human intestine, the bacterium E. coli, is one of the most-used tools of biochemistry. Wright found that an E. coli mutant, deficient in genes for synthesizing the amino acid, glycine, undergoes gene repair on addition of a normally-occurring biochemical (an alpha-hydroxy acid). The change, called by Wright "auto-adaptation," advances in seven sub-culturable steps, is population-wide, occurs over a predictable time, and is inhibited by the addition of glycine to the culture medium.

Another example in E. coli involves acquisition of new traits: Bartlett and Hinshelwood (the latter is President of the Royal Society of London) raised normal microbes in "sub-inhibitory" concentrations of streptomycin. Under these conditions, resistant cells did not survive or outgrow the sensitive cells; rather, whole populations became resistant. E. coli conjugates, i.e. cells unite followed by segregation of the different chromosome types; and in matings of trained and untrained cells, resistance was demonstrated to be genetic by segregation.

A fourth type of physiological gene change, of which several dozen instances have been reported in the past five years, finds one gene changing another. One striking example (Skaar, 1956), related to the Bartlett-Hinshelwood study reported above, also seems to involve whole-organism adaptive

response: The test organisms were E. coli sensitive to streptomycin which had present a certain gene "remotely linked" (widely separated on the same chromosome) to the gene giving inherited resistance. In sub-lethal antibiotic, genetic change to resistance was 100-fold greater than when the particular "mutator gene" was absent. Most gene-to-gene changes have been detected in corn. Finding that one gene regularly and specifically modifies another gene when they are together in the same cell, Brink (1958) regards 'mutation" as being inapplicable, and coins the new term "para-mutation".

Non-mutational (non-accidental) gene change in micro-organisms and somatic cells is not the same as somatic alteration of germ cells. However, notice these important-but-easily-overlooked facts. Many-cell organisms are not constantly many-cell; at the beginning of their life cycle, they more resemble protozoa or bacteria, and as migratory single cells they are exposed to their parents' biochemistry. At this stage they are "somatic-germ" cells, progenators of somatic as well as of germ lines. An organism's somatic genes are reproduced from the same genes (in the zygote) which give rise to its germ cell genes. The article by Helen Gay, "Nuclear control of the cell" (1960), and review papers by Ronald Fraser (1959) and the author (Morrill, 1958), describe protein flow and inter-action both inside and among cells which were undreamed of before the advent of the electron microscope and present radio tracer techniques. (Fraser's 1959 paper has an exceptionally good bibliography on the inter-dependence of all cell parts including genes).

What we seem confronted with is varying degree of exposure, degree of equilibrium, in the organism milieu, and not an absolute refractoriness on the one hand, an absolute response on the other. Germ cell genes are different because of their different position in the organism, and different time, their maturation and proliferation in the calm of the organism cycle. Conservativeness is not intrinsic in genes but is a function of the whole organism. Permanence is no more to be found in the organism than in the physical universe; biology seems to require a "field" and "relativity" concept as much as physics. Physiological gene change having been demonstrated in exposed cells only in the past decade, a breakthrough in the more complex somatic-germ change may be expected soon.

(3)

Organisms "come from" genes to a large extent, it is true--from the zygote at the beginning of the life cycle. But at the end, genes "come from" organisms, in the sex cells. Because germ cell genes preserve the basic patterns, it is no more necessary that genes circulate in the blood than that a person communicate by jumping through another person's ear into his brain. As articulated sounds express persons, so relatively simple molecules--such as hormone proteins--express somatic cell genes. The various gene structures are believed to be precisely articulated in proteins; proteins are formed on genes as a voice is formed in a man: "The DNA may be thought of as the master dies or templates from which proteins are manufactured" (Hoagland, 1959). Among men, sounds vibrated through air, or ink marks on a paper transmitted by light, are relayed as electrical impulses to the brain; there they are interpreted as things thought about or emotions felt by others. It is reasonable that within organisms there are as efficient mechanisms for relaying information about themselves to the most important molecules. On organ and tissue levels, the body is known to have tremendous interdependence.

To the writer, one of the half-dozen greatest discoveries ever made in biology appeared in Paul Weiss's 1952 paper, "Self-regulation of organ growth by its own products." The inter-cell plasma is thus revealed to include a pool of "cell type-specific" molecules secreted by the body cells (Weiss, 1955). Each tissue grows or not according to concentrations of its own coded substances in the master, pilot tissue. The plasma is the organism's Greenwich Clock, its Bureau of Standards; in this aqua regia, universal solvent, cells test their worth by dissolving tiny portions of themselves. Such a hormones-on-a-cell-level pool, regulating growth by homeostatic feed-back to every body part, seems just the mechanism needed by a modern theory of Pangenesis, or "Plasma-genesis," to apprise "like units already present, modifying them in accordance with the changes that have taken place in the periphery."

(C)

In the cases that follow, actual observation of somatic-germ transmission may be questioned for reasons we will note. That at least "physiological" change has occurred in metazoan germ genes seems to the writer as certain as in the preceding cases. But the first example, in rats, has been interpreted as exactly the reverse, i.e. mutation, by its authors as well as being so heralded by authorities generally. Asking the reader to see biochemical gene change in the celebrated Agar experiment is like asking him to believe Magellan's voyage really proved the world is flat. Therefore, I would like to be allowed to toss out the case in the manner "amazing if true," letting the reader decide.

In the 1920s and '30s, the British psychologist McDougall trained rats for over thirty generations to swim from a water tank to the more dimly lit of two exits. A steady reduction of errors over generations was attributed to "inheritance of acquired characteristics". Agar, Drummond and Tiegs (final report, 1948) repeated the experiment using controls. Both the training line (T) and the control line (C) of untrained stock were descended from a single pair of Wistar rats. Given six swims a day for 52 days after weaning, T rats and same-generation C line offshoots showed similar fluctuation; errors dropped, then rose sharply towards the initial level. (The fluctuation corresponded with a decline and improvement in husbandry, with vigor encouraging errors.) There was a problem, though; the training line rather consistently made fewer errors. The authors ascribe this to a "mutation" in the T line. However, T and C animals were not crossed, with the descendents then being tested for a special gene for aptitude for the test. Rather, the evidence was two other supposedly accidental changes revealed to be inherited by "mating tests". One modification resembled a mutation because associated with a particular gene locus, that of a "colour pattern gene". Let us dig into this a little.

The colour gene has three forms: $\underline{H}$ for fully pigmented, $\underline{h}^1$ for pigmented except for a streak on the belly, and $\underline{h}$, for pigmented head only. Since the "mutation" was from the less-pigmented $\underline{h}$ to the more-pigmented, and dominant, $\underline{h}^1$, it seems not to have been a mutation but an example of gene repair in metazoa such as we saw earlier in micro-organisms. Gene repair is also suggested on the basis of three earlier records of the same change in the same gene, a fact at variance with mutation's "accidental randomness". All four occurrences were in absence of added mutagens. Finally, that this was a case of gene repair (as different from mutation as being patched up in a hospital is different from being run over by a car) is indicated by its appearance in

the T line; as we shall see, a general inherited strengthening was going on in the T line. The other inherited change then, to which the authors devote three times more space, involved the training line's becoming "much larger and more vigorous" than the control line (Agar et al., p. 110).

Generation five T rats averaged 1.9 grams heavier than the C line animals; at generations 25-26, the difference was 12 grams (T rats 31% larger than C); by generations 34-36, the difference was 15 grams. This divergence was entirely in the T line; C animals remained at the average weight recorded by the Wistar Institute for its breeding stock. That this increase was genetic was shown by mating T males to C females: the 56 offspring were intermediate in weight. Breeding together F_1 animals demonstrated that "many genes were concerned" (Agar et al., p. 112), for no segregants appeared among 76 F_2 offspring. Regarding F_2 lack of variants, Agar et al. found it "surprising" that it did not exceed the parent lines, and "again surprising in view of the short time since the two lines were derived from the ancestral pair." That the phenomenon involved mutation I question for the following reasons:

1. Mutation is in single genes; Agar's effect involved coordinate modification in many genes.

2. The so-called mutations were beneficial, yet as Prof. Muller (1950) states, mutations are "well over 99%...harmful." (In multiple mutations, the 1000-to-1-more-likely harmful ones will swamp the helpful. In radiation experiments, single mutations are the rule because more than one is generally fatal.)

3. The change was continuous. "At about the 20th generation," wrote the authors, "the fact had gradually impressed itself upon us that the rats of line T were almost consistently much larger and more vigorous than those of line C." At this stage, the increase must have already been about 20%. (The weighing at generation 5 was for another purpose.)

4. The change was population-wide. In the preceding quotation, T rats were "almost consistently" larger. Even when aware of the phenomenon, and of the need to account for it by postulating a mutation, the authors did not observe sporadic increases in the T line. In the cross-breeding referred to above, offspring of 10 different T males were very uniform. Finally,

5. Changes arising from the "mutant" genes were identical with those from behavior. No increase was noted in the controls, but only in rats being handled, and challenged and exercised in the water tank, which showed "liveliness and eagerness when the cage was opened." The fact that inbreeding leads to increased variation (McLaren and Michie, 1954) suggests strong biological response under the conditions of the Agar experiment.

Perhaps the test could be repeated with controls for possible increase in weight due to handling, exercise, and even learning.

In concluding, I would like to suggest how Plasmagenesis seems to be working in a case now pressing towards solution, that of the effects of regeneration upon learning and retention in planaria. In the Agar test, transmission, if it occurred, involved a diffuse "well-being," perhaps a massive gene repair. In the case of the conditioned planaria, we are faced with the transmission of a specific characteristic; moreover, it seems the transmission of a behavioral

characteristic of the general type which we saw earlier underlying the phenomenon of animal selection, and seems to imply biochemical inheritance. Quite definitely the conditioned nerves, indeed the whole conditioned planarian, is altered chemically by the conditioning. The question is, how deep does the conditioning go?

Even for the unsophisticated flatworm, being cut in half must be a traumatic thing. As in protozoa at conjugation, bisected worms must undergo profound "de-differentiation" or "pooling" of specialized parts. Associated with eggs developing in ovaries are "nurse" cells whose function is to provide the ova with the basic sub-assemblies; similarly, specialized cells of regrowing planaria may be reduced to a nutritive function. In the absence of clear histological data on the problem, it is suggested that regeneration is not so much front halves growing tails and vice versa as it is whole new animals growing like embryos from germ cells.

Like every other multi-cellular organism, the planarian contains cells which are aloof from organism response or mutilation. Storer (1951) refers to "formative cells" in the loose meshwork between the planarian's muscles and internal organs; these cells "produce new structures in regeneration," and also give rise to the sex organs. The complete saving of the conditioned reflex after re-growth--the same as in uncut controls--indicates that formative cell genes are involved. In the simple, minute flatworm, the line between resting and working cells will be indistinct; formative cells are very unbuffered, more readily altered, compared with germ cells in, say, rats or humans. Conditioned cells secrete "cell type-specific" molecules; these induce modifications in formative cell genes such that their progeny contain the conditioning "chemically built-in".

"Sooner or later," wrote Darwin of Pangenesis, "some such view will have to be admitted." It is not beyond the realm of probability that Pangenesis and Darwin's other forgotten theory of Sexual Selection will in the long run prove the wisest of that wise man's several theories.

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Principles of Comparative Psychology

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McGraw Hill Book Co., N. Y., 1960. 453 pages, &7.95.

The Nature of Comparative Psychology.....Rolland H. Waters
Problems of Classification.....J. H. King and J. W. Nichols
Innate Behavior: The Organism in Its Environment
Sherman Ross and V.H. Denenberg
Sensory Processes.....Eckhard H. Hess
Biological Approach to Motivation.....Dorothy Rethslingshafer
Acquired Drives and the Curiosity-Investigative Motives
Robert A. Butler
Learning.....Austin H. Riesen
Complex Processes.....Arthur J. Riopelle
Comparative Social Psychology.....John P. Scott
Abnormal Behavior in Animals.....James A. Dinsmoor
Genetics and Individual Differences.....John L. Fuller
Behavior and the Nervous System.....J. V. Brady and B. N. Bunnell
Theoretical Foundations of Comparative Psychology
Willard E. Caldwell
Research Trends in Comparative Psychology...William R. Thompson

A Review

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A good text in any area of psychology is always welcome, but in the area of comparative psychology a new text answers a crying need. Adding to this the fact that the text here reviewed is a good one should convey my feeling that the publication of Principles of Comparative Psychology is an auspicious occasion.

Before discussing the volume itself, I wish to place it in the context of books now available to the teacher and student of comparative psychology, books with which the readers of the DIGEST are most probably familiar. The coverage of current experiments in psychology and ethology enables Principles to supercede both Stone and Maier and Schneirla as a general text, though it cannot replace the latter as to over-all scholarship and cohesiveness. It is a higher level text than Scott, and less specialized than Thorpe, Schiller, and Roe and Simpson.

As to the contents of Principles, the feeling one gets when reading the book is much like that derived from reading an Annual Review. The topics include the problems of definition, theoretical considerations, psychological and psycho-physiological matters. Each chapter is good in its own way. However, the aims of the various authors are obviously different, and it is here that the major difficulty of the book lies. Some authors attempt to cover entire subareas within comparative psychology, and thus can do no more than mention actual research. Others permit their chapters to focus on a few important experiments, and thus sacrifice the overview of the former approach. Each of these methods is legitimate. However, the inconsistency in type of coverage lends a certain uneveness to the volume which is regrettable, limiting its use as text and as reference work.

The strongest chapters in Principles are those concerned essentially with theory. It is in these chapters that the aims of the authors are most clearly defined. I wish particularly to cite King and Nichols' "Problems of Classification" as one which deals realistically with the questions which have been central ones for all true comparative psychologists. Caldwell's "Theoretical Foundations of Comparative Psychology" also should be recommended for the succint manner in which it reviews the main trends of the comparative area. However, the introductory chapter, "The Nature of Comparative Psychology", necessarily falls flat in its valiant but impossible stab at integrating too many approaches to the coverage of fields of comparative psychology. In addition, I feel that the area of "Early Experience and Behavior" warrants a separate chapter.

Another limitation to the multiple-author approach to textbook writing is that much excellent work tends to be lost in the partition. Thus, Howard Molz's work on the relationship between imprinting and avoidance learning, P. E. Field's beautifully systematic studies in homing behavior of salmon, R. W. Sperry's extensive experiments in the localization of learning, Knut Larsson's systematic work on sexual behavior in the male rat, to mention only a few important examples, have been lost along the way.

If most of the above comments sound negative, it is only because it is so much easier to criticize a book than to compliment it. I began by saying that we are presented here with a good book, and I am sincere in this judgment. Principles is a collection of many fine chapters. It provides for the teacher and student of comparative psychology a compilation of the most recent approaches and much recent experimental data in the area, and it should certainly serve as an excellent introduction to more specialized books on animal behavior.

Finally, if someone were to ask me the most pertinent question concerning my judgment of this book, "Will you use it in your course?", my answer would be, "I most certainly will." It is not the best of all books about the best of all worlds, but it is currently the best of all tries.

N.B. Since this issue (Volume II, Number 1) has been re-typed, the pagination varies somewhat from that in the original publication of the issue. Therefore Volume II, Number 2 follows immediately after this, although it begins with page 31.